

so often the case, the inferior arteries are the larger, the result is often disappointing; there is little or no clinical evidence of a fall in the activity of the toxic process or of favourable changes as estimated by basal metabolic and impedance angle tests. It must not be forgotten that, minor operation as it is, it has an appreciable mortality, and there is a by no means negligible minority in which the operation either aggravates the progress of the disease or at any rate entirely fails to check it.

The main value of ligation of the superior thyroid arteries is, I believe, as a test of the patient's susceptibility to surgical trauma, particularly in primary thyrotoxicosis associated with a large, intensely vascular goitre and great nervous excitability. If, as may happen, this trivial operation, conducted under local anaesthesia and occupying less than ten minutes, is followed by an intense post-operative reaction, then it clearly indicates that the major procedure would have been too hazardous.

At the Annual Meeting of the British Medical Association held in Edinburgh a few years ago, I heard the facile statement made that pre-operative iodine therapy had solved the problem of mortality in thyrotoxicosis. I did not believe this, nor do I believe that it can be solved entirely by the preliminary ligation of arteries.—I am, etc.,

London, W.1, July 21st.

CECIL A. JOLL.

Principles of Gynaecology

SIR,—I shall be grateful if you will allow me, on behalf of my collaborators—Mr. Datnow and Mr. A. C. H. Bell—and myself, to express our appreciation of the thoughtful and interesting review of the recent edition of *The Principles of Gynaecology*, which appeared in the *British Medical Journal* of July 14th (p. 65).

A textbook, your reviewer realizes, should be more than a sodden repetition of former views and methods, for science does not stand still: it should be not only a comprehensive and digested summary of our knowledge, but also an analysis and cinematograph of moving events. But what difficulties face those who undertake the task of incorporating intelligibly settled facts in a large tract of partly explored and ever-expanding territory! We feel, therefore, that, in saying our work will beneficially influence gynaecological thought, teaching, learning, and practice, your reviewer has said all we could wish him to say, for that has been our object from the first.

Your reviewer is curious to know why in this edition Part II (examination methods—previously Part III) is interposed between Part I (anatomy) and Part III (physiology—previously Part II). This is easily explained. Many functional conditions (Part III) require examination methods (Part II) for their elucidation. The anatomical phenomena (Part I) require chiefly dissectional methods.

To one knowing, as I do, the improvements which could be made, and which will be made in the next edition, your reviewer's choice of subjects for comment is a little surprising. Moreover, he seems sometimes to think that too much has been given, yet he asks for more! He writes:

"Here and there, however, broad principles are lost in detail. Thus the meaning and implications of the reduction of chromosomes during maturation are lost in describing the manner of their reduction. Neither among the possible causes of abortion nor under 'selective sterility' is there any reference to lethal gametic combinations, well recognized as cause of loss of zygotes in breeding experiments."

As a matter of fact, "the manner of their reduction" is not described at all, and brief mention only is made of reduction in respect of maturation of the ovum, and

of the relation of sex chromosomes to sex differentiation in *Drosophila*. It is presumed that the subject of genetics, which is of such fundamental concern not only to gynaecology but to all branches of medicine, is taught earlier in the curriculum. On the other hand, in regard to the clinical implications to which reference is made, I would point out that special attention is drawn to malformation of the embryo as a cause of early abortion; so, as everyone knows that this may be ascribed to "lethal gametic combinations," it can hardly be said that there is no mention of them. Besides, to have discussed selective sterility in the human subject in relation to the same factors would probably have been erroneous; we should have to go back to Ciona for a possible biological (but not chromosomal) explanation, with which I need not now trouble your readers. The one given is not unsound, and is of sociological importance.

With regard to the word "ponation," to which exception is taken, it is generally conceded that language is given to us to express not only our thoughts but also the facts of the physical universe, in which removal of the whole uterus from one place to another is an event well known to gynaecologists. It has no other name, yet it is of diagnostic importance. Why should it not be so identified to avoid confusion, not to create it? Anyway, "ponation" has appeared in every edition of *The Principles of Gynaecology*, including the first (1910), and has never before been challenged. We all know, of course, the past participle of *pono*, but are reluctant abstractly to "depose" the uterus. The Americans have, however, gone one worse, for they have quite recently introduced the horrid term "cession"; they speak of "retrocession" in this connexion.

Last, concerning the beginner's sense of proportion, in all seriousness I would ask whether he is likely to boggle at *three pages* only (excluding illustrations) on the important subject of hermaphroditism, or at *one and a half pages* on lead therapy? We hope that your reviewer's remarks, generous though they are, will not discourage the student from taking advantage of the high opinion we have of his desire to learn.—I am, etc.,

West Felton, July 17th.

W. BLAIR-BELL.

Tuberculin

SIR,—May I endorse Dr. Camac Wilkinson's request for an impartial investigation into the tuberculosis problem. Almost every day there is evidence in the medical journals pointing to a grave lack of co-ordination between the organizations dealing with the disease, and there would seem to be apathy prevailing at the centre of control. I understand that there is a department of the Ministry of Health which has the right of inspection of such organizations. If this is so such a department will presumably collect a good deal of valuable and irrefutable evidence of the value of certain methods of attack. Surely it should pass on this information to all local centres.

Two topical instances will illustrate my point. (1) The list of experts and others who, as the result of *ad hoc* research, give tuberculin a high value in their armamentarium grows more and more formidable, and yet after forty years there are Government centres for tuberculosis which ignore its use. (2) We are indebted to Dr. Hope Gosse and Dr. Erwin for recording the valuable advice that sunbathing may be prejudicial to the tuberculous diathesis (*British Medical Journal*, July 7th). This fact would appear to be contrary to public opinion as educated by the daily press, etc. I think it is pertinent to ask whether the appropriate department at the Ministry of Health has seen this article, whether it believes the